is seen in the case of the pea-crab and various bivalve molluscs and ascidians, or Myzostoma and Antedon. Now many jelly-fishes are infested with amphipods such as Hyperia, and it is a reasonable deduction that these crustaceans may be feeding on the food material collected by the jelly-fish. Thus, if the young fishes which take shelter below the umbrella of a jelly-fish assist the jelly-fish by keeping down ecto-parasites such as Hyperia, then an intelligible explanation is offered of the association of young fishes with such an apparently voracious host as a large jelly-fish, for in return the jelly-fish in these circumstances would have less of its own food stolen. J. H. ORTON.

Oyster Store, Packing Shed Island,
West Mersea, July 2,
and The Laboratory, Plymouth, July 14.

Roche's Limits for Satellites.

THE notice in NATURE (July 15, p. 89) respecting Dr. Fountain's work on Roche's limit for satellites brings to my recollection some estimates which I made many years ago with respect to the stability of satellites moving close to the surface of Mars (Trans. Roy. Dub. Soc., 1897, vol. vi.).

The question arose in connexion with a theory accounting for the "canals" as resulting from stresses set up in the surface rocks of Mars by the proximity of such satellites. The doubling of the canals came out nicely and the curvature of the canals as mapped by Lowell, Douglass, and Pickering was in agreement. But the doubt arose as to whether former satellites of sufficient magnitude could have preserved their stability when circulating around the planet with the requisite degree of approximation.

Assuming that the satellite possessed the cohesive strength of basalt and taking the case of Phobos supposed to be moving in an orbit but 23 miles from the planet's surface (*i.e.* with but five miles separating the surfaces of planet and satellite), I found that the satellite, even at this distance, would be stressed only to one-seventh of its breaking strength.

J. JOLY.

Trinity College, Dublin, July 15.

Optical Definition and Resolving Power.

IN Mr. Mallock's letter on "Definition, Resolving Power and Accuracy," published in NATURE of May 27 (vol. 109, p. 678), reference is made to the measurement of star images on eclipse plates, from which one might infer that the evidence for the Einstein deflexion of light obtained in 1919 was of a very doubtful character.

I have had no experience in measuring star images; but there is little doubt that if the same order of accuracy can be obtained as is possible in measuring spectrum lines, the Einstein deflexion should be easily determinable with a focal length of 19 feet, provided that it can be disentangled satisfactorily from the scale correction.

In my method of measuring photographs of spectra the image of a line is not bisected by a thread, but a positive copy is superposed on the negative in the micrometer and the coincidence of the two images estimated. By this means the intervals to be measured are doubled, and an extraordinary degree of precision is attainable with practice, as is shown by the agreement between different measurers. I have often had occasion to repeat measures made by one of my assistants of the shifts of the solar lines with reference to the arc lines, and we rarely differ by an amount exceeding 0·001 mm. in the interval measured. This is the result of taking the means of six settings in each line, and the probable error derived from the accordance of settings is usually about half a micron.

Probably star images cannot be measured so accurately as this by the ordinary method of bisection, but a skilled measurer should be able to determine the position of a star easily within 0·005 mm. or, on the scale of the eclipse plates, within $0''\cdot18$.

J. EVERSHED.

Kodaikanal, June 24.

Interspecific Sterility.

DR. BATESON in his letter on interspecific sterility (NATURE, July 15, p. 76) seems to lay insufficient emphasis on certain facts. If one considers plant and animal species in general, it would appear that interspecific sterility is by no means so general as was formerly assumed to be the case. Among the Œnotheras, in which great numbers of species crosses have been made, complete fertility, in the sense that large numbers of fertile offspring are produced, is the rule unless the forms differ in chromosome number. Even species of Œnothera which come from widely separated regions and differ conspicuously in all their characters, including flower-size, are fertile in crosses. That a certain amount of gametic and zygotic sterility also frequently occurs is of course well known, and it is probably correctly interpreted in terms of lethal factors. But lethal factors are not peculiar to wild species, for numbers of them arise in the mutations of *Drosophila melanogaster.*

Among animals, interspecific sterility appears to be more widespread, but even here the Bovidæ are, I believe, all interfertile. The contrasted condition of the Equidæ, at least as regards the horse and the ass, is accounted for by the difference in their chromosome numbers. In the Drosophilidæ, where interspecific sterility is extreme, there is a considerable range in chromosome form and number. The two species, *Drosophila melanogaster* and *D. simulans*, which are extremely alike and have similar chromosome groups yet produce sterile hybrids, might be cited as corresponding exactly to Dr. Bateson's conception of interspecific sterility. But it is an extreme case, and there are probably more numerous instances to cite on the other side.

Dr. Bateson refers to the case of *Œnothera gigas* and agrees that tetraploids frequently do not breed freely with diploids. But he says that "the applicability of that example is exceedingly doubtful" because we "can scarcely regard an unresolved pair of twins, such as the tetraploid must be, as a specifically distinct organism." It is this statement in particular to which I should be inclined for several reasons to take exception. In the first place, in calling the tetraploid an "unresolved pair of twins" Dr. Bateson scarcely recognises the intimate character of the union involved. I formerly analysed (*Arch. f. Zellforsch.* vol. 3, pp. 525-552, 1909) the changes which have occurred in *Œ. gigas* in so far as this could be done by comparative cell measurements, and found that the cell units were not merely larger, owing to the doubling in the chromosome content of their nuclei, but that in various tissues they were altered in shape, the increase in one dimension having been much greater than in another. Moreover, the genetic behaviour of *Œ. gigas* indicates, as de Vries first contended, that some other change has taken place in the germplasm of this species, in addition to the doubling of the chromosomes.

I have only recently been convinced on this point by comparisons of *Œ. gigas* with the tetraploid forms obtained by Winkler (*Zeits. f. Botanik*, 8,

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417-531, 1916) from his grafting experiments with the tomato. Here the tetraploid form apparently arises through the fusion of pairs of somatic nuclei where the cut surfaces of the cells are in contact. The forms obtained in this way appear to be merely tetraploid, stouter but without any alteration in the shape of leaves or other organs such as occurs in *Œ. gigas*. Winkler shows that these tetraploids have larger cells, nuclei and chloroplasts, but there are apparently no changes in cell shape except in the pollen grains, which have four pores instead of three just as in *Œ. gigas*. There seems no reason to alter my original interpretation of this change in the pollen grain as a direct result of the altered space relationships between the larger cell and its nucleus in the tetraploid forms.

It therefore appears probable that in some experimental tetraploid forms, such as Winkler's Solanums and probably the Marchals' tetraploid mosses, all the changes are such as follow directly from the original doubling of the chromosomes, while in others such as *Œ. gigas* and certain wild tetraploid species, an additional change has taken place in the germplasm. One cannot yet, however, regard this as fully proven.

It might also be pointed out here that the wide occurrence and evolutionary significance of tetraploid species in nature has not yet been generally realised by biologists. In Potentilla, for example, a whole group of wild species is tetraploid as compared with others. Species with $4X$ chromosomes are also known to occur in many other genera, such as Lactuca, Crepis, Muscari, Acer, etc. Such a doubling of the chromosome number must have occurred in connexion with the origin of many wild species and genera.

Tetraploidy undoubtedly forms a barrier to free crossing with diploid forms in any line of descent. I therefore see no reason why such forms as *Œ. gigas* arising in cultures should not be regarded as mutations significantly accompanied by partial interspecific sterility. For both in cultures and in Nature the cross-breds with unbalanced chromosome groups tend to be eliminated, leaving the pure forms each to perpetuate itself.

R. RUGGLES GATES.

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The Influence of Science.

IN his letter to NATURE (July 15, p. 78) Sir George Greenhill does well in directing attention to the fact that the quarrel between Galileo and the Holy Office was largely a domestic quarrel between two opposing schools of thought. It is historically a fallacy, though a very common one, to suppose that the freedom of experimental inquiry was secured in consequence of the action of the Roman Curia in the case of Galileo. So long as scientific investigators confined themselves to their own legitimate subjects of study, and left doctrinal and Scriptural matters alone, freedom of experimental inquiry was never interfered with by ecclesiastical authority. Nicholas Copernicus was a devout Catholic priest, and his heliocentric doctrine was freely taught, even in ecclesiastical colleges, until Galileo interested himself as a champion of the system.

For Galileo, as all historians testify, was a truculent and hot-headed controversialist, who, in spite of the advice of his friends not to raise the question (I quote from his contemporary Guicciardini, the Tuscan ambassador), " demanded that the Pope and the Holy Office should declare the Copernican system to be founded on the Bible; he wrote memorial after memorial. Paul V., wearied with his importunities, decreed that the controversy should be determined in a Congregation, and having sent for Cardinal Bellarmine, ordered him to bring it immediately before the Holy Office." Let me make use of an analogy from the practice of our own English Courts to elucidate the matter. The Court of King's Bench was originally constituted to judge cases pertaining to the King's peace. To widen its jurisdiction, all classes of injuries, even actions for breach of contract, had to be interpreted as acts of violence, even though perpetrated by otherwise peaceful citizens.

Similarly, the Holy Office had, by its procedure, to consider every case submitted to it, with reference to heresy and orthodoxy. Let us also bear in mind that Holy Scripture must ordinarily be interpreted literally, unless a rigid proof can be adduced to the contrary. What rigid proofs, in the then state of scientific knowledge, could Galileo allege in support of his contention that the Copernican system was founded on the Bible? Let us recall in passing that such eminent men of science as Tycho-Brahe and Bacon rejected the system, while Descartes would not admit the hypothesis as proved, and that the Cardinals of the Congregation, so far as proofs of Copernicanism were concerned, had perforce to rely upon the opinion of their scientific advisers. The only proofs that were brought forward were the analogy of Jupiter's satellites, the moon-like phases of Venus, and the simplicity with which the theory accounted for the observed motions of the planets. The other alleged proofs from the tides and the earth's magnetism were worthless.

On the other side was the apparent authority of the words of Scripture, the universal experience of mankind, which seemed to attest that the earth was immovable, while the sun, moon, and stars moved round it, and the Ptolemaic system which for centuries had explained in a satisfactory manner the apparent movements of the planets. In such circumstances what could the Congregation do but declare, according to the forms of the court, that the Copernican system was heretical, in the sense that it contravened the literal and obvious meaning of Scripture? According to the knowledge of that time, the Copernican system was " false and absurd philosophically, inasmuch as it expressly contradicts the doctrine of Holy Scripture." The proviso and restriction is noticeable.

What did Galileo do? Instead of teaching the Copernican theory as a scientific hypothesis, after a most generous reception in Rome in 1624 by his friend Pope Urban VIII., he returned to Florence, and in his famous Dialogue not only lampooned his benefactor, but was guilty of gross contempt of court. For such egregious contempt of court an English judge would rightly commit a man to prison. Galileo had as a penance to recite certain prayers, and was sent to a beautiful villa at Arcetri, where, free from the disturbing influences of controversy, he was at liberty to pursue his favourite studies.

As Whewell says in his " History of the Inductive Sciences," pp. 425-6, " The persecutors of Galileo are still held up to the scorn and aversion of mankind; although, as we have seen, they did not act until it seemed that their position compelled them to do so, and then proceeded with all the gentleness and moderation which were compatible with judicial forms." Or, to quote another non-Catholic, Prof. A. De Morgan (English Cyclopædia, " Motion of the Earth "), " We heartily wish all persecutions, Catholic and Protestant, had been as honest and as mild." Nor did the Roman Curia possess a monopoly in opposition to Copernicanism. Martin Luther went further, for he considered Copernicus to be an arrogant fool, who wrote in defiance of Scripture, while Melancthon declared

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